

article to a recommendation. If the use or disuse of parentheses is reduced to a recommendation, confusion will result. The deletion of the date from the citation of a specific name does not convey any particular information, but the deletion of parentheses can be misinterpreted to represent a specific name in its original combination.

#### REFERENCES

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I do not agree with the proposal of Drs Gagné, Thompson and Knutson. Parentheses are a useful indication and I have never found them 'expensive and time consuming', neither in preparing my *Fossilium Catalogus* on Carboniferous and Permian trilobites, nor in preparing my volume on multituberculates. Article 51c should stay unchanged, in my opinion.

#### COMMENT ON THE PROPOSED AMENDMENT TO ARTICLE 70b

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(see vol. 41, pp. 156–158)

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I do not believe that the illegal behaviour by entomologists that Sabrosky seeks to justify by amending the Code does, as he asserts, maintain stability and universality of nomenclature, since any subsequent author may well find reasonable grounds for disagreeing with some of the assumptions behind the behaviour. Moreover the notion of recognition of the species actually involved seems to me of doubtful validity and unlikely to produce stability.

2. If an author Smith establishes a new genus *X-us* with designated type species *A-us b-us* Jones and in the same work describes as *X-us b-us* (Jones) certain specimens now held not to belong to that species, it does not necessarily mean that he has misidentified the type species of *X-us*; he may have misidentified his specimens as *A-us b-us* Jones, quite a different matter, or, commonly, he is less of a splitter than later authors who take the view that his described specimens represent a

new species. He may very well genuinely have wished Jones's *A-us b-us* to be the type species.

3. Sabrosky's proposal recalls the case of *Calycoceras* Hyatt, 1900, dealt with in Opinion 557. Hyatt had designated as type species of his new genus a nominal species that he attributed to an author who had wrongly described specimens as identical with the species described by the original author of the specific name. I had asked the Commission to rule that the nominal species of the original author should be the type. Commissioner Mayr argued that it was important that 'the zoological concept which the author of a new taxon has for its type be regarded by subsequent authors'. He also referred to the 'erroneous belief of many authors that a name is the type of a genus and not a zoological object'. Fortunately the Commission, Mayr and two others dissenting, agreed to the terms of the application. Of course a name as such is not the type of a genus, but no more is a zoological object; the type is a nominal species. Any attempt to replace this notion with that of zoological object or even zoological concept can only, in my view, lead to permanent confusion.

4. Suppose that a subsequent author, faced with the situation in para 2 above, believes that both *A-us b-us* Jones and three other species included by Smith in *X-us* are congeneric but that the specimens described, wrongly, by Smith as *X-us b-us* (Jones) in fact belong to a different genus. Here, if we followed Sabrosky's interpretation of 'the species actually involved' we could well be running counter to 'the zoological concept' of Smith. One can easily imagine several other types of confusion that could arise if the only objective criterion, the species named by the designator, is abandoned.

5. My response to the Secretary's appeal, therefore, is to argue that Article 70b is wrong in principle and dangerous in practice and should be expunged. If, however, a majority of zoologists cannot accept the simple and logical rule that the type of a genus is a nominal species designated by the original or a subsequent author, and still hanker after the idea that it is a specimen or a zoological concept, then Article 70b might be redrafted as follows:

- (b) If a person considers that the author of a genus incorrectly identified specimens with the nominal species that he designated as type species, or that he included, and which was subsequently so designated, and if the genus has subsequently been widely interpreted as if the type were the nominal species to which the misidentified specimens belong, the person is to continue to regard as type species the designated nominal species, but may, if he believes that serious instability of nomenclature would be caused by maintenance of the designated type species, apply to the Commission to, etc.

6. Article 70b has already moved too far from principle. It would be deplorable if improper behaviour such as that quoted in para 3 of Sabrosky's proposal were to be validated. The type of a genus is a nomenclatural concept, not a zoological one. Hence it is right that the 'meaning' of a genus depends on a nomenclatural concept of which the type species is the name-bearer. The 'meaning' of a genus should not be treated as the zoological concept that Sabrosky and Crosskey think that an author had in mind when he designated a (nominal) type species, and also described species that he then held to be congeneric; it is this notion that can lead, at least potentially, to variable results and hence to instability.

7. While the nomenclatural concept is objective, in accordance with the collective provisions of the Code, the zoological concept is subjective and therefore variable and cannot be pinned down by the Code. Only the type concept plus the definition of nominal taxon give hope of stability.